

Incentive Effects of Equity Compensation: Employee-level Evidence from Google¹

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We examine whether differences in stock exposure motivate differences in performance using data from Google. Employees with more initial exposure to Google stock than peers hired at the same time and job grade perform slightly better across a variety of measures. To attempt to determine the direction of causality in this relationship, we use the fact that Google assigns option strike prices based on the stock price during an employee's first week to identify variation in initial stock exposure that due to luck rather than negotiation. We find that employees who begin work when the Google stock price is lower than surrounding weeks (and thus receive more initial stock exposure) do not perform better than their peers. In contrast, we find evidence that employees do respond to incentives tied to individual performance, such as those created by the promotion cycle. We also find that unvested equity compensation may indirectly affect performance by encouraging retention, since employees appear to make retention and performance decisions jointly.

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Incentives Effects of Equity Compensation: Employee-level Evidence from Google

A commonly cited justification for employee equity compensation is that exposing an employee's wealth to the firm's stock price will cause the employee to work harder or smarter. Indeed, "incentive stock options" is the name used in federal tax law for a tax-favored category of options. In the recent debate over whether corporate accounting should treat employee stock options as an expense, managers of some firms argued that options' incentive effects are crucial to companies' success.

For example, Ed Paradise (2004) of Cisco Systems argues that "companies in technology, pharma and bio related industries know that employees work harder when they own a piece of the company through option grants" and that "there is no greater regulatory threat to our industries today" than being forced to expense stock options.

At the same, others have questioned how well equity-based compensation can motivate non-executives and other employees whose individual performance may have a limited effect on the stock price. Some scholars have treated the popularity of granting stock options to rank-and-file employees as a puzzle to be explained. Oyer (2004), entitled "Why Do Firms Use Incentives That Have No Incentive Effects?" argues that unvested options have the attractive feature of providing a retention incentive that varies with firm performance, helping retain workers when the firm and its industry are successful. Lazear (2003) and Oyer and Schaefer (2005) add sorting as an alternative story: firms may include options in job offers to attract employees who are optimistic about its prospects.

Existing empirical analysis of the various rationales for employee stock options is largely based on between-firm comparisons using publicly available data. For example, Core and Guay (2001) find that firms that grant more options to their employees also had more options exercised in the same or prior year, and argue that this is consistent with firms desiring to maintain employee incentives. Oyer and Schaefer (2005) find that firms with greater own-stock and industry volatility make more heavy use of options and argue that this is consistent with the sorting and retention rationales for options.² Sesil and

² A related literature seeks to "test" the incentives versus risk tradeoff in the standard agency model (Holmstrom, 1979) by examining whether firms with greater stock volatility make less use of options. Evidence is mixed. For example, while Aggarwal and Samwick (1999) find a negative correlation between volatility and option usage, Core and Guay (2002) find the opposite relationship. Prendergast (1999, 2002) argues that the latter result is more common in the literature and argues that high volatility firms may systematically differ from low volatility firms: specifically, they may operate in more uncertain environments that require greater use of incentives. More generally, Prendergast's argument highlights the difficulties in interpreting cross-firm evidence.

Lin (2005) conduct a within-firm analysis, finding that firm performance improves after executive stock options are granted and interpreting this as evidence of options having an incentive effect.³

In this paper, we use employee-level data from a single firm to test the performance effects of differences in stock exposure at that particular firm. This study is in that sense more in the spirit of Lazear (2000), Shearer (2004), Freeman and Kleiner (2005), and Bandiera, Barankay, and Rasul (2007) who examine the effects of changes in piece-rate incentives on production workers' productivity within a specific firm.

In both between and within-firm studies of incentives, correlations between firm characteristics and incentive usage or between incentives and performance need not imply causal relationships. The within-firm studies have addressed this issue by either studying sharp discontinuities in incentives (Lazear; Freeman and Kleiner) or varying incentives as part of a field experiment (Shearer; Bandiera, Barankay, and Rasul). Our approach is to exploit the fact that the strike prices of employee stock options are always (in Google's case) set at the market price of the stock on the day of the grant, and the day of the grant is determined by the day an employee starts work. Options with lower strike prices provide greater exposure to the firm's stock price performance, and therefore should provide greater incentives. An employee who starts work when the stock is temporarily low gets a lower strike price -- and thus more exposure to the stock -- than an employee who joins when the stock is temporarily high.

We use "strike price luck" as a plausibly exogenous source of variation in option-provided incentives, comparing the performance of lucky and unlucky employees hired into the same job grade in the same quarter. Across a variety of measures, including formal performance evaluations and measures of hours worked and activity while at work, we find no statistically significant evidence of options-provided incentives inducing higher performance. When we examine the correlation between the size of an employee's initial equity grant and their subsequent performance, we find a very modestly sized positive relationship that is significant in most specifications. If one assumes that endogeneity will cause this correlation to overstate the causal effect (due to more options being offered to or attracting more capable employees), then this estimate provides an upward bound on the incentive effect that is in some cases tighter than that provided by the strike price luck measure.

³ Bergstresser, Desai, and Rauh (2006) also document firm performance improving after executive stock option grants but provide an alternative explanation, finding evidence that earnings manipulation via assumed rates of return on pension assets contributes to the improvement.

In addition, we ask whether employees' evaluated performance and activity declines after options and restricted stock vest or are exercised or sold. Most initial equity grants at Google vest gradually over the course of four years, and we find a gradual decline in relative performance after an employee's initial equity grant is fully vested. We find that the positive correlation between initial grant size and performance disappears after four years. We also find that exit rates climb (from very low levels) after an employee's initial option grant fully vests, and that the correlation between performance and vesting/exercise is primarily accounted for by employees who eventually exit. That said, among employees whose equity has vested, those who have exercised or sold do not perform worse.

These results suggest that employees jointly make performance and exit decisions. Unvested equity encourages retention, and policies that encourage retention may have an indirect effect on performance. That said, even if we interpret the correlations between vesting and performance as a causal effect of reduced equity exposure on performance, the implied cost of improving performance via equity compensation is extremely high.

In contrast to the minimal incentive effects of options and restricted stock, we show that promotions, which are more directly tied to individual performance, do have incentive effects. Both evaluated performance and activity are higher in quarters when promotion decisions are made; for evaluated performance these effects are statistically significantly stronger among employees who are eligible for promotion. This helps confirm that the weaker results for equity compensation are not due to Google employees being immune to extrinsic motivation or to our data being too noisy to capture performance changes.

The downside of examining a single firm is that it need not be representative of other firms in the economy. Indeed, Google is probably more unrepresentative than many firms one might have chosen, albeit in some sense this increases the interest in these results. Due to Google's broad-based option grants prior to its Initial Public Offering (IPO) and subsequent stock appreciation, its pre-IPO non-executive employees ended up with extremely high levels of equity exposure. This makes our finding of modestly sized incentive and retention effects of equity compensation among pre-IPO employees all the more surprising. One might be concerned about offsetting wealth effects, but we find consistent results among post-IPO employees with lower average levels of Google-equity wealth.

The next section provides some background on the role of equity in employee compensation at Google, how employee stock option strike prices are determined, and how some employees come to have “lucky” strike prices due to unpredictable movements in the price of Google stock. It also tests whether employees with lucky and unlucky strike prices differ in any way correlated with observables that would suggest selection effects. The following section presents an analysis of the relationship between an employee’s satisfaction with her job and compensation and her initial salary, option and stock grants, and strike price luck. The fourth section repeats this analysis for employee performance, while the fifth section examines physical measures of employee effort, activity, and output. The sixth section examines changes in performance after equity vesting or exercise. The seventh section examines the performance effects of eligibility for promotion and compares the cost of performance improvement implied by our estimates for promotion and equity-based incentives. A concluding discussion follows.

II. Background

As many readers will be aware, Google stock has appreciated considerably during its history. Google was founded as a private company in 1998 and began trading publicly on August 19, 2004. For tax-related and other reasons, firms almost always issue employee stock options with strike prices equal to a company’s current stock price (Hall and Murphy, 2000), and Google followed this practice before and after its IPO. Prior to Google’s IPO, it set option strike prices equal to an internal estimate of its market value. This estimate was constant from Google’s founding until early 2003, after which it was revised upward approximately once per month (Figure 1). These revisions were prompted by changes recommended by an outside expert to reflect Google’s increased value. After Google’s initial public offering (IPO), the stock price was set in the public market (Figure 2).

Employees’ option strike prices were determined by the stock price on the day their options were granted, which was usually within a few days of when they started work.⁴ As a result an employee who

⁴ Stock option grants for new employees occurred at the next meeting of the relevant committee following their hire date. Hire dates are usually on Mondays. After the IPO, almost all employees received their option grants on the Wednesday either 2 or 9 days after their start date. Prior to the IPO, the exact timing of grants did not affect their strike price so long as they were issued before any stock price change, and therefore the start date determined the strike price. We therefore use start dates before the IPO, and grant dates after the IPO, to determine whether an employee was lucky or unlucky with respect to joining right before or after a stock price change.

started at Google right before an increase in Google's stock price received a lower strike price than one who joined right after. The former employee's options were both more valuable and had higher deltas. The delta of an option is the first derivative of its value with respect to the underlying stock price; this ranges from zero to one for an option and is decreasing in strike price. All else equal, options with a higher delta create a greater link between employee wealth and firm performance and thus should have stronger incentive effects.⁵ We use as our measure of strike price luck the difference between the option strike price received by an employee and a moving average strike price from a 4, 9, 13, or 26-week surrounding period.

As one can see from Figures 1 and 2, the window length usually does not affect conclusions about whether a strike price was lucky or unlucky. An example of an exception would be employees who received grants in mid-November 2007, when Google's stock price was approximately \$630, below the 4, 9, and 13 week averages of \$665-680 but above the 26-week average of \$610. We find below that strike price luck with respect to the 26-week average is the best predictor of satisfaction with one's compensation, and so focus on this measure in our subsequent analysis, but our results are not very sensitive to this choice.

Stock options are, of course, only one component of compensation. In addition to generous non-cash benefits such as food and Google-provided shuttles,⁶ full-time Google employees receive salaries, stock and option grants, and annual and occasional bonuses. Annual bonuses, quantitatively the more important, are determined by a formula and are a function of one's salary, a job-specific target bonus percentage, a multiplier that depends on company-wide performance, and a multiplier that depends on individual performance. Individual performance is measured quarterly: written evaluations are collected from an employee's manager, peers, and direct reports as part of a "360" evaluation and these evaluations are translated into a single number. Annual bonuses depend on the average of these four quarterly evaluations and average approximately 10 and 40 percent of base salary for bottom and top-decile performers, respectively.

⁵ Option deltas are the standard measure of the exposure to a firm's stock performance provided by employee stock options. For example, see Chidambaran and Prabhala (2003)'s analysis of changes in deltas after option repricing.

⁶ See, for example, the articles accompanying *Fortune* magazine's "Best 100 companies to work for" lists in 2007, - 2012.

Table 1 provides data on the relative size of different compensation components in typical initial job offers received before and after the IPO. We are interested in discussing the relative importance of different compensation components rather than the level of compensation, and so chose a “typical” salary level of \$100,000, which approximates the mean salary of \$98,160 for “Computer and Mathematical Science Occupations” in the San Jose-Sunnyvale-Santa Clara MSA for May 2007 in the Bureau of Labor Statistics’ *Occupational Employment Statistics*. To avoid disclosing information Google deems proprietary, we report only very approximate figures.⁷ We report figures separately for offers made during the pre-IPO period (January 2003 to June 2004) and post-IPO period (October 2004 to December 2007).⁸ Data on stock and option grants include grants made within 90 days of hire date and exclude “refresher” grants, which are typically smaller than initial grants, are made a few years after an employee is hired, and whose size depends on how an employee has performed at Google.

We can make four observations. First, unsurprisingly, pre-IPO employees ended up with significantly greater exposure to Google stock than post-IPO employees. Indeed, granting enough options to post-IPO hires to match the stock exposure of their pre-IPO colleagues employees would be quite expensive. Second, restricted stock became more common after the IPO. Restricted stock accounts for about one-third to one-half of post-IPO employees’ stock exposure, compared with less than 3 percent before the IPO. Third, the role of stock and option compensation in total compensation actually increased in the post-IPO period, at least if stock and options are valued at grant date. Particularly in the pre-IPO period, it is quite possible than employees who joined Google believed that expected future Google stock returns were strongly positive and that expectations of future returns were lower after the IPO. Therefore, a smaller stake in the firm, as valued at the time, might have been sufficient to attract employees.⁹ Fourth, the average performance bonus as a percent of salary remained relatively constant after the IPO.

⁷ Figures at this level of approximation can also be inferred from Google’s disclosures about its employee stock options in its Form S-1 and 10Ks.

⁸ In this analysis and those that follow, we begin the “pre-IPO” period in January 2003 for two reasons: 1) option strike prices did not vary before early 2003, and thus there was no “strike price luck” prior to that date and 2) employee performance data is unavailable before 2003. The IPO occurred in roughly the middle of 2004Q3; we exclude this quarter from both pre and post-IPO periods.

⁹ Core and Guay (2001) find that firms with positive recent stock returns are more likely to grant options. They argue that one possible cause of this relationship is employees overvaluing equity compensation due to an expectation of momentum in returns. Bergman and Jenter (2007) point out that since optimistic employees can also purchase their employer’s equity at market prices, there are unlikely to be gains to public companies from paying optimistic employees in equity rather than cash. Before Google’s IPO, however, most employees would

The data in Table 1 can be used to put strike price luck in context. Table 2, Panel A reports that pre-IPO employees who received their grants before a strike price increase (pre-IPO strike price increases were always implemented over the weekend) received strike prices that were about 14 log percentage points below the four-week moving average, while those who joined the week of the increase received strike prices that were 14 log percentage points higher.

Making some reasonable assumptions about intended holding period and implied volatility, the “lucky” employees received options with a delta that was 8 percentage points higher than their colleagues joining on surrounding dates (0.69 versus 0.61), while their “unlucky” colleagues received an option delta that was 8 percentage points lower.¹⁰ For the “typical” pre-IPO employee in Table 1, who received 5,000 options, a lucky employee would experience an increase in wealth per \$1 increase in Google’s stock price that was \$800 (16 percentage point difference in delta times 5,000) more than an “unlucky” employee. Put another way, a lucky employee with \$44,000 in options and an unlucky employee with \$57,500 in options would both have the same exposure to Google’s stock price as an employee with \$50,000 in options and average luck. Of course, as Google’s stock appreciated and pre-IPO options moved well into the money, the difference between pre-IPO strike prices became less significant, since the deltas of all of these options were very close to one.

Table 2 also reports tests for whether some employees were able to time their start dates successfully in order to receive better strike prices. Timing one’s start date should have been impossible unless future employees received inside information about future internal stock price revisions (before the IPO) or stock-price-relevant news (after the IPO). We find no statistically significant evidence that more employees joined during periods of strike price luck.

As an additional test, in Table 3 we examine whether strike price luck is correlated with employees’ observable characteristics. We test whether, controlling for an employee’s quarter of hiring and initial job track and level, employees who received higher initial salaries or larger options grants were more likely to receive lucky strike prices.¹¹ We find no statistically significant evidence of this, either for pre or

have only been able to receive its shares through grants, and so it is conceivable that they might have valued these shares more highly than their grant value due to an expectation of positive returns.

¹⁰ This calculation assumes annualized implied volatility of 35 percent (the average for the post-IPO portion of our sample as calculated by OptionMetrics) and a holding period equal to the four year vesting period.

¹¹ Full-time employees at Google are assigned to one of six tracks: technical (T), operations (O), sales (SD or SI), exempt (E), non-exempt (N), or executive (X or VP). We include the first four tracks in our study and exclude

post-IPO hires. We also find no evidence that employees with lucky strike prices were more sought after by Google, as proxied for by the average score they received in their pre-employment interviews.¹² For a subset of employees, we also use the results of an April 2006 survey to test whether strike price luck was correlated with variables that might proxy for prior connections at Google or knowledge of the mechanics of stock options at startups (prior work experience, English fluency, or having a Stanford or Computer Science degree). We again find no evidence that certain employees received luckier strike prices.¹³

Having established the plausible exogeneity of strike price luck, we turn to testing whether it affects job satisfaction and satisfaction with one's compensation.

III. Equity grants, strike price luck, and job satisfaction

In this section, we examine correlations between job satisfaction and an employee's initial salary, stock and options grants, and strike price luck. In September 2006, full-time employees were asked to complete a survey asking them to rate their satisfaction with various aspects of their jobs on a 5 or 7-point scale. There were eleven categories, one of which was satisfaction with compensation and benefits (see Cowgill and Zitzewitz, 2008, for more detail on this data). In Table 4, we regress normalized satisfaction scores on the logs of initial salary, log of shares of stock and options initially granted, and the difference between the log strike price received by the employee and its moving average from the surrounding weeks. We find a positive and significant relationship between satisfaction with compensation and both strike price luck and the size of initial equity grants. The strongest partial correlation is with the measure of luck relative to the 26-week average. Using this measure, a one standard deviation difference in strike price luck (about 12 log percentage points) is

executives and non-exempt (hourly) employees. Non-executive employees are also assigned to one of nine levels (1-9); a track and level together define a job grade (e.g., T4 for a software engineer at level 4).

¹² In addition to providing qualitative feedback to the hiring committee on a candidate, interviewers give each interviewee a single numeric score. Cowgill and Zitzewitz (2008) describe this data in more detail. The variable in the regressions in Table 3 is the average score from a candidate's interviews conducted at Google's offices (phone screens and campus interviews are excluded), normalized for successful candidates within job grade*month combinations.

¹³ The tests in Tables 2 and 3 focus on our four-week measure of strike price luck, since that measure should have been the easiest to predict using inside information. We also found no evidence of correlations for our longer-term measures.

associated with satisfaction with compensation that is 0.036 standard deviations higher. A large initial grant of stock and options is also associated with higher satisfaction.

Interestingly, despite the fact that the survey was conducted in September and October 2006, when Google stock was trading over \$400 per share, there was a significant correlation between strike price luck and satisfaction for both pre and post-IPO employees. This is surprising given that even a large percentage difference in the strike price of a pre-IPO option (whose median strike price was \$10/share) would amount to a small difference in the value of the options in 2006. One possible explanation is that pre-IPO employees formed an impression of the fairness of their compensation at a time when these strike price differences mattered more, and this impression persisted. Unfortunately, we do not have job satisfaction data from before 2006, and so cannot directly test this explanation.

Table 5 repeats these tests for overall job satisfaction and for individual questions about compensation satisfaction from the survey. We find the correlation between satisfaction with compensation and strike price luck is stronger for questions about the fairness of one's compensation and for questions asking about satisfaction with long-term total compensation (salary, bonus, stock, options, and benefits) over 1, 1-2, and 3-5 year horizons. The correlation is weaker for questions about satisfaction with salary and about comparisons of compensation with competitors. The size of initial stock and option grants is also correlated with satisfaction, and the pattern across questions is fairly consistent.

In general, the results for these individual questions are consistent with the size of equity grants and strike price luck being noticed by employees and with it affecting their impressions of how well and how fairly they are compensated. In the next two sections, we ask whether they have a detectable effect on their performance and their level of activity at work.

IV. Equity grants, strike price luck, and performance

In this section we ask whether employees with greater exposure to Google's stock performance from their *initial* grants of stock and options work harder. We focus on initial grants to avoid having our results affected by high -performing employees receiving subsequent grants. We conduct this and subsequent analysis comparing employees who were hired in the same quarter and into the same job grade (i.e., the same job track and level). A Google employee's exposure to the stock price at a given

time from their initial stock and option grants is given by the number of shares of stock they were granted s , plus the number of options granted o times the delta of those options at that time d_t , i.e. $s+o*d_t$. We can rewrite the log of this as:

$$\ln(s+o*d_t) = \ln(s+o*MA_t) + \ln[(s+o*d_t)/(s+o*MA_t)],$$

where MA_t is the delta of options with the moving average strike price from the period surrounding the employee's option grant. The first term is the size of the employee's initial grant in shares, with the options multiplied by what the employee's delta would have been had she received the moving average strike price. The second term accounts for the difference in delta due to strike price luck. For employees whose options have appreciated significantly, deltas will be close to one, and the variance in the second term will be very small.

Table 6 presents regressions of quarterly employee performance ratings (normalized within job grade and quarter combinations) on the first and second term above, the log of initial salary, and fixed effects for quarter hired*initial job grade combinations.¹⁴ Option deltas are calculated as of the beginning of the time period in which performance is being measured. Standard errors cluster for the month of the initial options grant, to account for correlations in performance of employees hired at the same time, since option delta differences and numbers of shares granted will also be correlated within these groups of employees. Employees who received larger initial grants performed better, particularly among post-IPO hires. Estimates of the correlation between performance and options exposure through strike price luck are less precise and not significantly different from either zero or from the coefficient on shares granted.¹⁵

The coefficients on strike price luck and the size of the grant can be directly compared, given that the variables are defined such that they add to yield an employee's log exposure to Google stock. There is reason to expect the correlation between initial stock and option grants and performance to yield an upwardly biased estimate of the causal effect of options, since one would expect employees likely to

¹⁴ We have also estimated these models with the first term decomposed into the log of total shares granted and the weighted average delta of the stock and options. Since the latter component is very similar for employees hired at the same time, its coefficient is very imprecisely estimated and is never different from zero or from the coefficient on the first component. Coefficients on the strike price luck term are quite similar, however.

¹⁵ The exception to this statement is that the coefficient on the delta difference due to strike price luck is large and just significant for pre-IPO employees during the post-IPO period. We should note that the variance of this term in that subsample is very small (the standard deviation is 0.90 percent) and as a result this coefficient is quite imprecisely estimated.

perform well to be offered more equity compensation and to be differentially attracted by equity compensation.

If we view the top of the confidence interval as providing an upward bound on the incentive effects of options, in most specifications a tighter bound is provided by the grant size coefficient than the strike price luck coefficient. For example, in the regressions including all employees, the coefficient on grant size is positive and significant, the coefficient on strike price luck is statistically insignificant, and the upper confidence interval is lower for grant size than for strike price luck. The regression that is limited to post-IPO employees is the exception, where the correlation between performance and equity grant size is positive and significant, but the correlation of performance and strike price luck is negative and nearly significant at the five percent level.

The best possible case for the incentive effects of options that can be made from Table 6 would be to take the upper confidence interval for the post-IPO employees. This upper bound, if the likely upward bias is ignored and it is interpreted causally, implies that an increase in the size of an employee's equity grant by 1 percent would increase her performance by 0.149 percent of a standard deviation. For the typical post-IPO employee profiled in Table 1, to have done this on grant day would have cost approximately \$1,000. Google stock appreciated during most of our post-IPO sample period, so the ex post cost of increasing grants by one percent would have been higher and, likewise, the ex-post incentives effects of increasing grants should have been greater than would have been anticipated on grant day. These estimates suggest that, even if the most optimistic estimates from Table 6 are used, options are an expensive means of providing incentives to employees, as will be even clearer when we present our estimates of the cost-effectiveness of promotion-based incentives below.

V. Equity grants and measures of activity

In addition to analyzing employee performance evaluations, we can also test whether equity incentives have effects on employees' activity while at work. Table 7 provides an overview of the activity measures we were able to develop for two types of employees: software engineers and online sales and

operations (OSO) staff. Software engineers develop and maintain Google's software; OSO staff provide assistance to Google's online advertisers.¹⁶

For both groups of employees, our measures capture only a subset of their job responsibilities. We therefore limit our subsamples to those engineers and OSO staff for whom this problem is the smallest. For software engineers, we exclude those working in Product Management, managers and directors, and those with certain job titles (hardware, project, and product engineers) that we were advised would be poorly captured by our measures. For online sales staff, we limited our sample to the most junior staff supporting the AdWords and AdSense products. OSO staff at junior levels tended to deal with easier customer issues and refer more complicated ones to more experienced employees, as a result there is more homogeneity in the amount of work that a particular unit of activity represents than there is for higher-level OSO staff. We have data for software engineer activity from January 2006 to March 2008; our data for OSO staff is from July 2004 to March 2008. Taken together, these subsets of software engineers and OSO staff we include account for approximately half of full-time employee*months during the time periods in question.

For software engineers, we have data that capture many of the tasks that are central to their jobs: writing code, reviewing their peers' code, fixing bugs, building (i.e., compiling) software for testing, editing the internal wiki that documents Google's code, reading the wiki, and interviewing job applicants. The measures are described below:

Code reviews. Before new code becomes part of Google's official code base, it must be reviewed by one or more peers. These reviews are fairly regular; the average engineer participates in just over one per workday. We count the number of code reviews an engineer participates in as an author and the number as a reviewer. The former is a proxy for the amount of new code written, while the latter is a measure of one's helpfulness as a reviewer and the extent to which one has been assigned responsibility for maintaining an important part of Google's codebase.

¹⁶ OSO is described in more detail in Groyberg, Thomas, and Wagonfeld (2008). Most OSO staff support either the AdWords (advertising on Google search queries) or AdSense (ads Google feeds to third-party websites such as blogs) products, and to limit heterogeneity, we restrict our sample to these employees.

Bugs database actions. Bugs at Google are tracked by a database. Employees make entries as they identify, diagnose, and fix bugs. Software engineers are most likely to be involved in diagnosing and fixing bugs, while non-engineers often identify them.

Builds. Software is “built” primarily for testing purposes, and also to use the software in an internal or external environment. While performing a build is clearly work-related activity, a software engineer who performs a lot of builds for every unit of code they complete may be working less efficiently than one who performs fewer builds.

Perforce (P4) calls. Engineers make calls to the Perforce system, a third-party software program that maintains Google’s codebase and facilitates engineers interaction, for a variety of purposes, including when they check out code for editing or viewing, when they submit code for review. As with builds, an efficient engineer may accomplish a given task using fewer P4 calls than a less efficient engineer.

Wiki page edits and views. Google’s code is documented in an extensive internal wiki. When significant changes are made to code, the engineer responsible often updates the wiki. Providing documentation of changes is viewed as good citizenship. As such, editing the wiki is the type of behavior we’d expect to see in employees concerned with the long-term functioning of the company as a whole. Stock options are intended to elicit this form of “ownership mentality” more effectively than incentives on individual output (Kerr, 1975; Holmstrom and Milgrom, 1991).

Job interviews conducted. Given its rapid growth and selective hiring process, there is a significant need for engineers to participate in candidate interviewing. This is arguably another citizenship behavior that would be under-provided in an incentive scheme focused on individual performance.

In the most quantifiable part of their job, OSO staff review ads submitted by online advertisers, respond to customer service representative (CSR) emails, and make suggestions to advertisers to optimize their ad copy to make them more effective. As OSO staff gain experience, their activity mix tends to shift from ad approvals to CSR emails and optimizations, and the share of their activity that is not well captured by these measures also increases. Our data include counts of the number of CSR emails sent

and ads approved, as well as counts of the number of page views in the ICS software through which ads are approved and optimized.

With each of these measures, there is considerable heterogeneity in the amount of output a unit of activity represents. Heterogeneity is probably greatest for measures such as P4 calls, builds, and wiki ICS page views, but is still considerable for code reviews and CSR emails. Some activities are sometimes automated or done in batches and we can detect this using time stamps. For p4 calls and ad approvals, we count multiple calls or approvals in a given five-second period as one. This is likely closer to a measure of true output than counting, for example, fifty similar ads approved at once as fifty times the work of approving a unique ad. We also windsorize outliers, limiting activity measures for an employee*months to the 99th percentile of the distribution of all employee months with a positive amount of activity, since these outliers are likely to reflect automated activity rather than extremely high levels of output.¹⁷ Even with these modifications, our activity measures are still better thought of as capturing a combination of effort and output, as opposed to output alone.

In addition to counts of activity, we also use the timestamps of the more frequent forms of activity to measure the number of hours during a work month in which some work activity occurs. This averages 5.2 hours for software engineers and 7.1 hours for OSO staff. Since these employees work longer hours than implied by these measures, clearly there are some hours during the day in which they work but do not perform activity captured by our measures.¹⁸

Table 8 examines the correlation between employee's performance evaluations and their activity measures. Employee quarters are sorted into deciles within job grade*quarter combinations, and the table reports the average value for each measure, normalized within job grade*quarter combinations. For software engineers, code reviews, bug actions, and interviews conducted are the most highly correlated measures, and the relationships appear monotonic. Hours with activity and wiki edits also have positive and monotonic relationships. Builds rise with performance for below median performers only, suggesting that a low number of builds may be correlated with low performance, but a high number of builds may be indicative of working inefficiently. Correlations between performance and

¹⁷ Conclusions are not sensitive to variations in this cutoff (e.g., 95th percentile, 99.5th percentile).

¹⁸ We considered using the difference between the first and last time-stamped activity during a day as a measure of hours worked, but rejected this as being too prone to distortion by variations in employee's work style (i.e., working continuously for shorter hours versus interspersing work and leisure throughout the day) that could be correlated with the variables of interest.

both p4 calls and wiki views are close to zero. The heterogeneity in the output-activity ratio problem discussed above is likely most extreme for p4 calls. Wiki views are positively correlated with performance within employees of a given tenure at Google, but wiki views decline with experience while performance increases.

For OSO staff, the correlations between activity measures and performance are generally lower. CSR emails are the most correlated; this is partly because our sample includes the most junior OSO staff, and the best performing among them are more likely to experience a shift in workload toward work that results in an email. Ad approvals, ICS page views, and hours with an ICS page view are positively correlated with performance but the relationship peaks at decile 7. The best performing OSO staff work on harder problems that lead to slightly lower measured activity.

Table 9 presents a multivariate analysis of the relationship between our activity measures and performance. We omit builds from the regression since those data begin only June 2007.¹⁹ With a few exceptions, each of these measures remains positively correlated with performance. For the reasons discussed above, wiki page views are negatively correlated with performance across software engineers of multiple tenure levels, but positively correlated within a tenure level. ICS page views are negatively correlated in models that control for ads approvals; this may indicate that employees who produce many page views but few ad approvals may be working less efficiently.

It is important to note that the explanatory power of our activity measures for an individual employee's performance is low, as indicated by R-squareds of 0.089 and 0.109 for software engineers and 0.012 and 0.066 for OSO staff. While these measures may be informative about the activity of large groups of employees, such as those with high and low options exposure or lucky or unlucky strike prices, they are likely to be too imperfect a measure of output to be of much use in evaluating individuals.²⁰

¹⁹ If we include builds, the sample size drops by approximately 40 percent, the estimated coefficient on builds is close to zero and statistically insignificant, and the other coefficients are less precisely estimated. Conclusions about their relative role in econometrically explaining performance are unaffected, however.

²⁰ Furthermore, evaluating programmers based on, e.g. number of code reviews participated in or lines of code written, is likely to have perverse effects. Google generally does not use raw productivity data to evaluate individual employees, as it expects most employees to make significant contributions in ways that cannot be easily quantified. The data we're using mostly comes from Google usage logs for its productivity tools. Google saves these logs in order to maintain and optimize its internal productivity systems. Among other things, the data is used for planning capacity for the tools, assessing the impact of feature changes in the tools and identifying groups affected by changes in the tools. From our conversations with managers at Google, the problems of both noise and incentive distortion from using quantitative measures are both well understood.

Tables 10 and 11 present estimates for the models in Table 6, with activity measures as the dependent variable. Table 10 presents the results for software engineers. The first column in Table 10 presents replicates the regression from Table 6 for all employees for the time period we have activity data (January 2006 to March 2008); the second column estimates the same regression for our sample of software engineers only; the third column uses as a dependent variable the Perf score predicted by the activity measures and the model in Table 9. Subsequent columns use individual activity measures as dependent variables.

The results are fairly consistent. Engineers who receive larger initial equity grants both earn higher Perf scores and engage in more activity, but those who received greater options exposure via lucky strike prices did not behave significantly differently. The results for individual activity measures are largely consistent, although the coefficient on grant size is statistically significant only for the two code review measures and is negative and significant for participating in interviews. Table 11 presents analogous results for OSO staff. As with the software engineers, the coefficients on equity grant size are positive and often significant for performance and activity, and while those on strike price luck are usually not significantly different from either zero or from the coefficients on grant size.

One concern we had was that the weak relationship between equity incentives and performance may be due to offsetting wealth effects. Options may be providing incentives, but, especially at Google, they may also be providing employees with wealth levels that reduce work effort. In Table 12 we divide the sample into quartiles based on beginning of quarter Google-equity wealth.²¹ Our measure of option exposure due to strike price luck is never significantly correlated with performance. The positive correlation between initial equity grant size and performance is largest for employees with second-quartile wealth levels. For employees in the top quartile, who are the only ones with equity wealth that is a non-trivial percentage of their lifetime income, this correlation is no longer significant. That said, excluding the top quartile increases the coefficient estimate by only about 20 percent, suggesting including the wealthiest employees in our sample is not causing us to significantly underestimate the incentive effects of options.

²¹ We calculate Google-equity wealth as the immediate exercise value of an employee's initial option and stock grants. We use immediate exercise value rather than Black-Scholes value since we judged the former to be a more likely proxy for the perceived wealth of an employee with limited attention. Results are quite similar if we use Black-Scholes instead, however.

To summarize, the analysis of activity measures yields results that are consistent with those for performance evaluations. Employees who received larger initial grants performed slightly better, but those who received more options exposure for plausibly exogenous reasons (i.e., through strike price luck) did not. We now turn to asking what can be learned from employee's performance around the vesting and sale or exercise of their granted equity.

VI. Vesting, equity sales, and performance

As mentioned above, the typical equity grant at Google vests gradually over four years. The typical schedule is for one-quarter to vest on the grants first anniversary, and then for $1/48^{\text{th}}$ to vest each month until the four-year anniversary. The "typical" pre-IPO employee in Table 1 who received a 5,000 share grant would have just over 100 shares vesting each month in years 2-4 of employment. For an employee who joined in 2003, by the time of the fourth anniversary, these options would have been worth about \$500 per share. Leaving before the fourth anniversary would therefore mean forfeiting over \$50,000 per month. Unsurprisingly, the exit rate is low before the fourth anniversary, and increases thereafter, albeit to levels that are still well below industry norms.²²

Figure 3 plots exit rates and normalized performance evaluations by quarter since the initial equity grant.²³ As mentioned, exit rates increase after the fourth anniversary. Relative performance is low for the first two quarters, increases sharply and then peaks around quarter 6. Relative performance then declines gradually, with the decline accelerating slightly after quarter 12. Figure 4 repeats the same plot for software engineers. Most activity measures follow the same pattern of increasing sharply in the first few quarters, peaking around quarter 6, and then declining gradually.²⁴

It should be mentioned that performance and activity measures are normalized within a quarter and job grade, so when we find that relative performance declines after quarter six, this means that quarter-six employees perform better than quarter-sixteen employees who are in the same job grade. If our

²² The average monthly quit and separation rates for professional and business services from 2003-7 in the Bureau of Labor Statistics' *Job Openings and Labor Turnover Survey* was 2.2 and 4.5 percent, respectively.

²³ The values on the y-axes on this graph have been redacted. We hope to be able to share more in a later version.

²⁴ One exception is Wiki views, which peak in an employee's first quarter and decline rapidly. This is unsurprising given that new software engineers need to look at the Wiki frequently to learn about Google's codebase.

sample were limited to the lowest job grade, then this result would reflect the fact that the quarter-sixteen employees were by construction those who did not get promoted in four years. For this reason, we do not construct a version of Figures 3 and 4 for our sample of junior OSO staff. Our software engineer sample excludes managers and engineers in non-programming roles, but the incidence of leaving the software engineer sample through a promotion or job transfer is low enough that including these software engineers would not affect the qualitative conclusions from the figures.

That said, even for the software engineers, promotions can influence these results. As we will discuss further below, Perf scores typically decline in the two quarters after promotion. This is partly by design, since Perf scores are relative to expectations, expectations are higher after a promotion, and employees and evaluators are told that a post-promotion decline is normal. If we correct for this phenomenon, by examining the performance-tenure curve controlling for recent promotions, we find a steeper increase between quarters 1 and 6 and that the decline in relative performance begins later (around quarter 8) and is more gradual.

Table 13 presents regressions that test for performance declines after initial equity grants vest and are sold. The regressions predict relative performance using the size of the original equity grant, and the log of the proportion of shares granted that remain unvested and unsold (and unexercised in the case of options). Since the log of percentage close to zero can be a large negative number, when the unvested/unsold share is less than 10%, we set the $\ln(\text{unvested/unsold})$ term equal to $\ln(0.1)$ and turn on an indicator variable for a low unvested/unsold share.

The estimates in Table 13 find a large effect of vesting but, interestingly, no relationship between equity sales and performance. The vesting effect appears non-linear – the regression coefficients in many specifications imply that the difference between 90 percent and 100 percent vested is greater than the difference between 0 and 90 percent vested.

In addition to relative performance declining in employees' fourth years, we also find that the relationship between grant size and performance also declines. Table 14 examines how the correlation between initial equity grant size and performance declines with tenure. The first column replicates the first column of Table 13, and subsequent columns limit the sample to employees with a certain tenure at Google. The coefficient of performance on grant size is largest in the first year, declines in the second

and third year, is no longer statistically significant in the fourth year, and is negative (but not statistically significant) in subsequent years.

The combination of increased exit rates and declining performance suggests that employees may jointly make exit and performance decisions. One story consistent with the results is that performing well at a firm has non-pecuniary returns, such as more interesting job assignments or the opportunity for advancement, that are only realized if an employee remains at the firm. Given Google's stock price increase, unvested options created so-called "golden handcuffs" that made exiting the firm unattractive. Employees who know they will stay at Google for a certain amount of time have a greater incentive to invest in performing well today in order to earn firm-specific career returns later.

Another, related, story is that employees make the decision to exit a firm well in advance of their actual exit. Once an employee is planning to exit, the return to performing well at one's current firm declines, while the return to job search and networking outside the firm increases. By motivating retention, options may have an indirect effect on performance more important than their direct incentive effect. Perhaps in recognition of this dynamic, Google grants "refresher" option grants to employees whose options have vested. Of course, given the increase in the stock price, these grants are much smaller in shares than the initial grants they are replacing.²⁵

VII. Promotions and performance

Since late 2004, almost all promotions at Google have occurred as part of a six or nine-month promotion cycle. There are six in our sample period: at end of 2004Q4, 2005Q2, 2005Q4, 2006Q2, 2006Q4, and 2007Q3. These "promotion quarters" are announced in advance. It is rare for an employee to be promoted in two consecutive cycles, so we can consider employees who were not promoted in the last cycle to be eligible for promotion in the current cycle. We can examine how employees respond to promotion-based incentives by either comparing their behavior in promotion quarters and non-

²⁵ We did not examine estimates of the correlation between performance and having received a refresher grant. The interpretation of this correlation would be even more problematic than the correlation of performance and initial stock grants, the firm observes much more information with which to predict the future performance of its current employees, and one would expect the size of refresher grants to be related to expectations of future performance.

promotion quarters, or by comparing the behavior in promotion quarters or employees who are eligible or not eligible for promotion.²⁶

Table 15 presents regressions of performance and activity measures on indicator variables for prior promotions, for being in a promotion quarter, and for interactions of the two. The regressions also include the logs of initial salary and initial equity grant size and fixed effects for the number of quarters since the initial grant to control flexibly for tenure effects. In the first two columns, performance is normalized within job grade*quarter combinations as in the earlier analysis. As a result, average normalized performance is identical in promotion and non-promotion quarters, and so this term is omitted from the regressions. In the other columns, performance is normalized within job grade*year combinations, which allows promotion and non-promotion quarters to be compared. These latter regressions include quarter of year fixed effects to control for seasonal variation. The fact that the promotions were done in Q3 in 2007 rather than Q2 and Q4 allows us to separate seasonal effects from promotion quarter effects.

In both types of model, we find that Perf scores are lower for employees who were promoted in the last two quarters, most likely due to higher expectations in their new jobs. Controlling for this, Perf scores are higher in promotion quarters, especially for employees who are eligible for promotion (i.e. those who were not promoted recently). Put another way, newly promoted employees receive lower Perf scores, and this is especially true in promotion quarters.

These results could reflect employees who are eligible for promotion working harder, especially in promotion quarters. Alternatively, if managers seek to maximize the rate at which their employees get promoted, they may grade eligible employees more highly at the expense of employees who are ineligible by virtue of a recent promotion. Turning to the activity measures can help distinguish these stories. We find that software engineers engage in more activity across all measures except Wiki edits (a citizenship behavior that might get deprioritized when incentives for individual performance are

²⁶ Employees should only work harder in promotion quarters if they believe that their performance right before promotion decisions will have more influence on promotions. While this belief seems intuitive, we tested it using regression that predicted promotions using either evaluated performance or activity measures from the current and two prior quarters (and controlled for job grade*quarter fixed effects). In both types of regressions, we found that the coefficient on current-quarter performance was significantly larger; if we interpret this regression causally, it suggests that employees are indeed justified in working harder in promotion quarters.

higher). The effects are generally larger for employees who are eligible for promotion, although this last difference is statistically significant only for the hours with activity measure. Taken together, these results suggest that employees do work harder during promotion quarters, but that the fact that Perf scores increase more for those eligible for promotion may also reflect strategic grading on the part of managers. Interestingly, the activity measures suggest no post-promotion decline, suggesting that the decline in Perf scores is indeed due to changes in performance expectations.

These results suggest that Google employees do respond to the incentives for individual performance created by the promotion process, by working harder during promotion quarters. This result suggests that our earlier results suggesting a minimal response to larger equity grants were not due to Google employees not responding to extrinsic motivation, or to our measures failing to capture employee performance.

Unfortunately, comparing the costs of providing incentives via equity grants and promotion directly is not straightforward, since equity grants may have other benefits (retention, selection) and promotions likewise have organization costs and benefits that are difficult to quantify. Furthermore, it is difficult to infer the incentive effect of promotions from the difference in performance between promotion and non-promotion quarters without knowing employees' expectation of the relative weighting of recent performance. That said, comparing the estimated effects in Table 15 with those in Table 10, we find that the performance difference between promotion and non-promotion quarters is larger than the estimated effect of a doubling in size of equity grants. From an incentive perspective, rewards tied to individual performance appear to be more cost effective, although the decline in Wiki edits during promotion quarters is consistent with these worry that individual incentives discouraging citizenship behavior (Kerr, 1975; Holmstrom and Milgrom, 1991).

VIII. Conclusion

In popular discussions of employee stock options, incentive effects are given prominence. Our results suggest that this prominence may be undeserved, and that the rationale for employee stock options likely lies elsewhere. We find that employees who receive greater exposure to Google stock via lucky strike prices do not perform better across a variety of measures. The correlation between employee's

initial grant size and performance, which likely yields an overestimate of any causal relationship, is also quite small.

In contrast, there is evidence that equity compensation and its vesting affects retention, and that employees may make joint decisions about retention and performance. We find that exit rates are very low until employee's fourth anniversary, when initial equity grants are fully vested. Exit rates increase at that time (albeit to levels that are still low relative to other firms), and employees' relative performance also declines gradually around their fourth anniversary. While Google's stock appreciation led vesting to create an incentive for retention that would be too expensive for most firms to replicate, these results suggest that unvested equity helps encourage retention, and encouraging retention has the additional benefit of avoiding declines in performance in advance of employee exit.

Finally, we examine the performance and activity changes that accompany promotions. All employees perform better and engage in more activity during promotion quarters, and these effects are generally larger for those who are eligible for promotion. Recently promoted employees earn lower performance evaluations, consistent with higher expectations in their new roles, but actually engage in more activity. This suggests that extra effort exerted during the promotion window does not come at the cost of reduced effort once employees are promoted. These results suggest that our findings of limited incentive effects for equity compensation are not driven by a lack of responsiveness to incentives in general. They also speak to the relative efficacy of incentive schemes linked to individual and group performance.

These results are obtained using data from only one company, and Google is unrepresentative of the technology industry and the broader economy on several dimensions. Many of those dimensions arguably make our study more interesting rather than less, however. Google has experienced more stock appreciation than almost any other firm, and thus its employees have equity exposure that few companies would seek to recreate. Yet we find modest effects from differences in these levels of equity exposure, which suggests that differences may be likewise limited in most other firms. In contrast, Google has a flat organizational structure and anecdotal evidence suggests that it is a relatively pleasant place to be a junior employee.²⁷ If the promotion process generates incentives at Google, it would likely do so at a more hierarchical firm as well.

²⁷ See, for example, the aforementioned Fortune magazine "Best 100 companies to work for" articles.

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Figure 1. Strike prices for newly granted options, Jan 2003 to IPO (8/19/04)

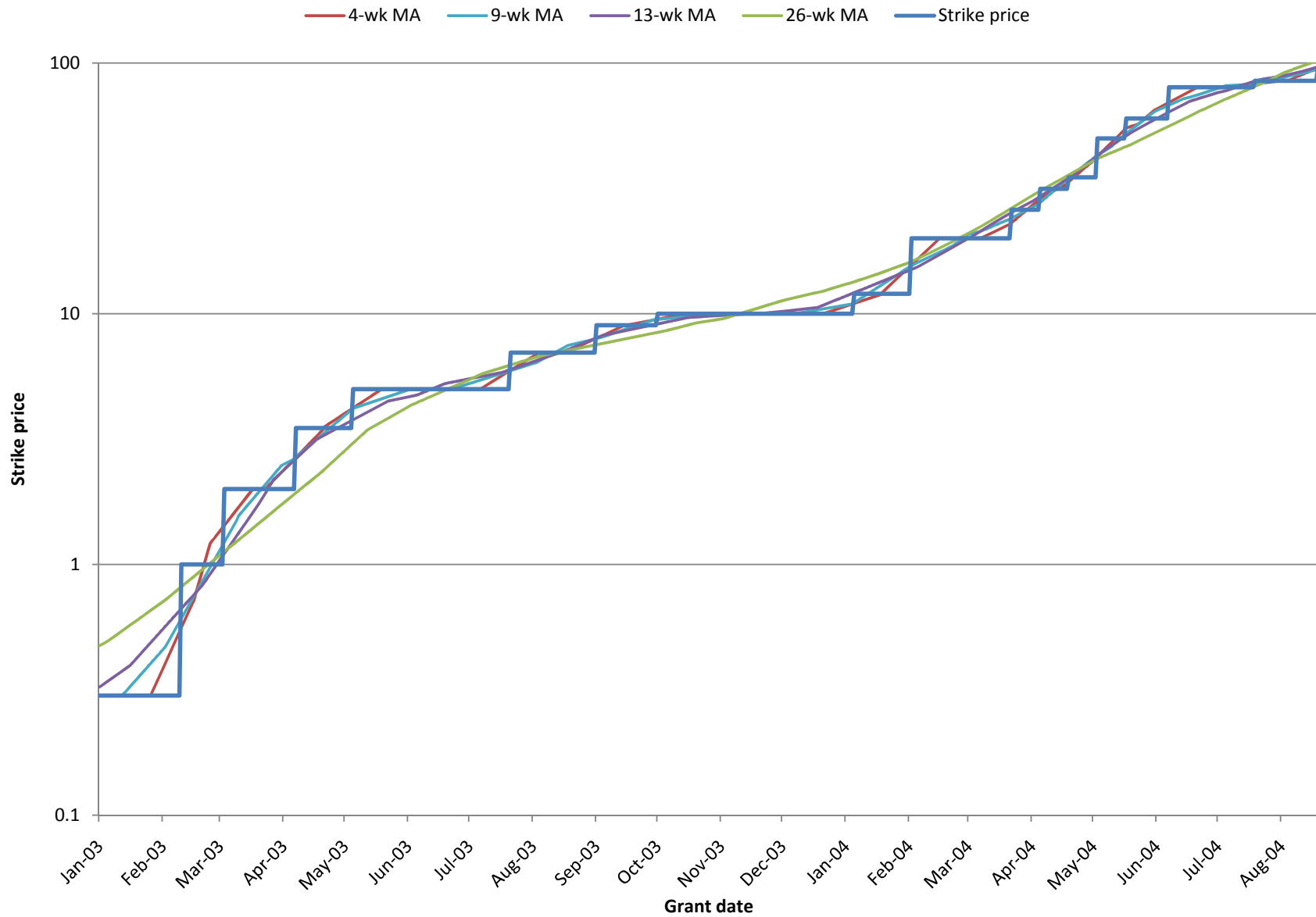


Figure 2. Strike prices for newly granted options, Aug 2004 to June 2008

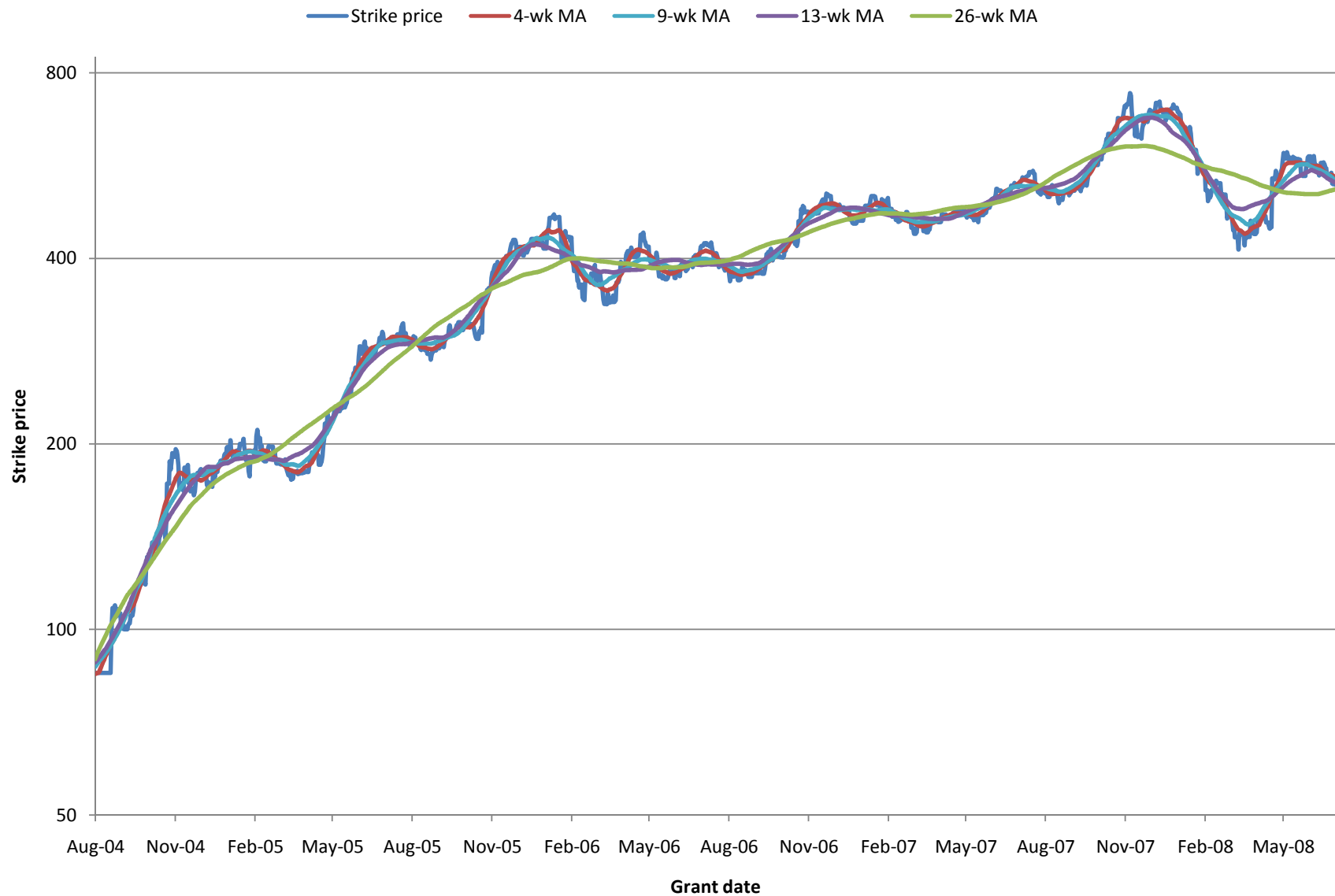


Figure 3. Option vesting, performance, and attrition -- all employees

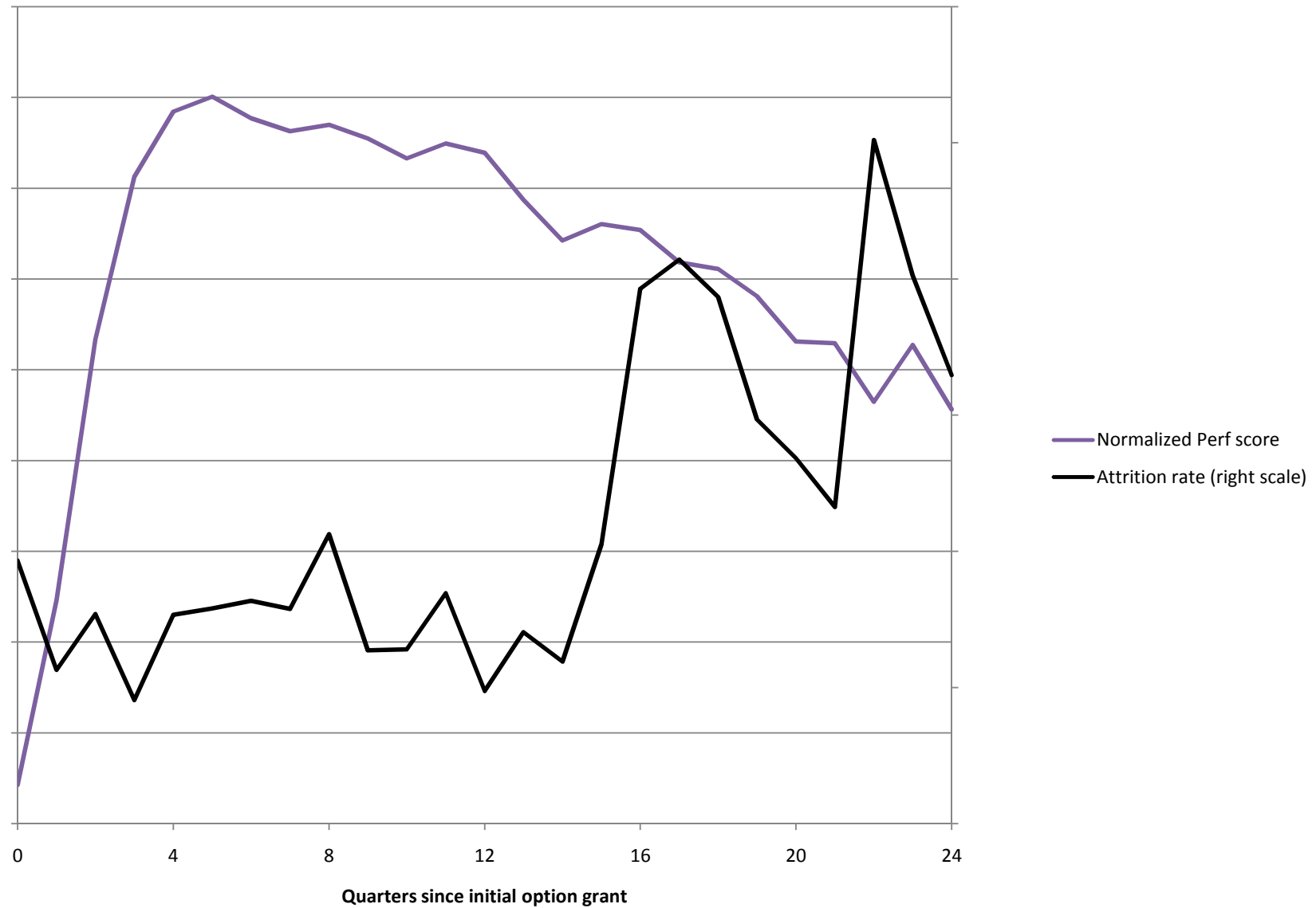


Figure 4. Vesting, performance, and attrition -- Software Engineers

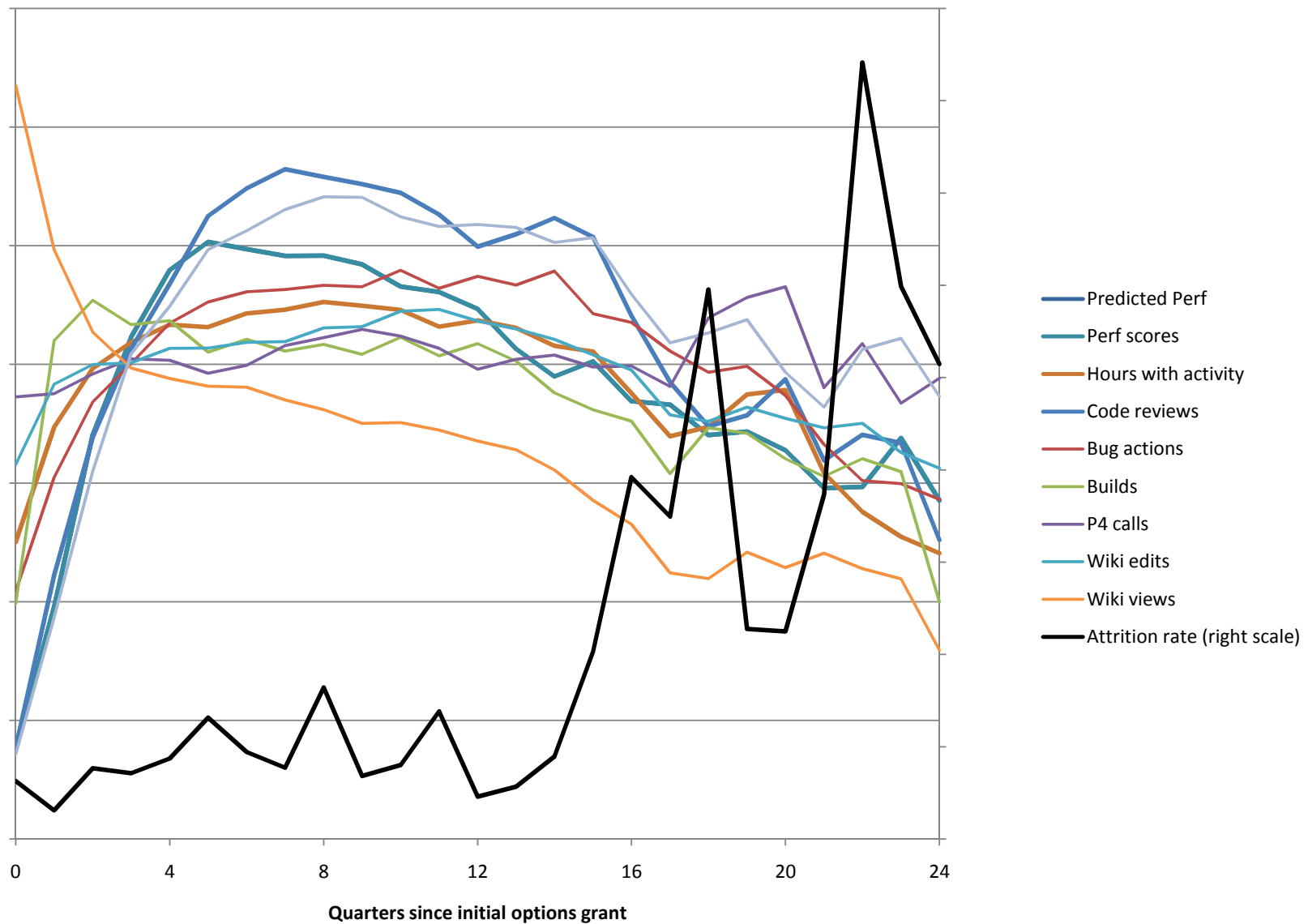


Table 1. Typical initial offers for employees with \$100,000 salaries

	Pre-IPO period (Jan 2003 to June 2004)	Post-IPO period (Oct 2004 to Dec 2007)
Initial salary	\$100,000	\$100,000
Average bonus as percent of salary	20%	20%
Option grants (made within 90 days of hire)		
Percent receiving	100%	90%
Options granted	5,000	200
Notional value on grant day	\$50,000	\$100,000
Stock grants (made within 90 days of hire)		
Percent receiving	20%	90%
Restricted stock granted	100	100
Value on grant day	\$1,000	\$50,000

Note: In order to avoid disclosing data Google deems proprietary, these figures are rounded off. They should be viewed as approximate and are presented only to facilitate the discussion of approximate order of magnitude of, e.g., the cost of a hypothetical increase in equity compensation. We hope to be able to share more precise summary statistics in a later version.

Table 2. Option price changes and start dates

Panel A. Hires before and after 15 strike price increases in pre-IPO period (January 2003 to June 2004)

Time period	New employee starts	Strike price luck
		Ln(Strike price) - four-week MA
Two weeks before increase	18.0	-0.032
Week before increase	29.3	-0.137
Week of increase	20.7	0.138
Week after increase	16.5	0.039

Panel B. Predicting weekly hires using recent and future log strike price changes

Dependent variable: new employees receiving option grants in each week

	Pre-IPO (Jan 2003 to June 2004)		Post-IPO (Oct 2004 to June 2008)	
	OLS	Poisson	OLS	Poisson
Ln(strike price) weekly change (Friday close to Friday close)				
2 weeks after grant date	-2.4 (7.3)	-0.2 (0.4)	3.6 (70.9)	0.0 (0.9)
Week after grant date	15.5 (18.5)	0.6 (0.6)	-44.5 (72.2)	-0.6 (0.9)
Week of grant date	-0.8 (8.5)	-0.1 (0.4)	-101.8 (66.7)	-1.3 (0.8)
Week before grant date	-9.5* (5.1)	-0.7* (0.3)	-60.3 (68.3)	-0.8 (0.8)
2 weeks before grant date	1.4 (8.9)	0.1 (0.3)	-22.3 (66.7)	-0.3 (0.8)
Constant	19.3*** (2.9)	3.0*** (0.1)	82.3*** (4.1)	4.4*** (0.0)
Observations (weeks)	75	75	204	204
R2 or pseudo R2	0.045	0.039	0.015	0.012

Each observation is a week. Standard errors are heteroskedasticity robust.

Table 3. Correlation between strike-price luck and employee characteristicsDependent variable: $[\text{Ln}(4\text{-wk moving average strike price}) - \text{Ln}(\text{strike price of employee's initial options grant})] * 100$

Pre/post-IPO sample	Both	Pre-IPO	Post-IPO	Both	Both	Both
Excludes observations without survey data					X	X
	(1)	(2)	(3)	(4)	(5)	(6)
Ln(Options in initial grant)	0.001 (0.144)	1.428 (0.876)	-0.086 (0.136)	-0.101 (0.137)	0.354 (0.328)	0.317 (0.348)
Ln(Initial salary)	0.028 (0.125)	-2.718 (1.627)	0.016 (0.098)	0.125 (0.098)	0.208 (0.495)	0.203 (0.527)
Normalized average interview score				-0.015 (0.019)	0.077 (0.099)	0.072 (0.099)
Pre-Google employee experience						
Grad degree?						-0.683 (0.924)
Years of pre-Google work experience						-0.048 (0.037)
Self-reported English fluency (1 = limited, 5 = native)						0.248 (0.275)
Stanford degree (undergrad or graduate)						-0.684 (0.594)
Last degree was in Computer Science?						-0.048 (0.487)
Fixed effects						
Initial job grade	X	X	X	X	X	X
Quarter of initial options grant	X	X	X	X	X	X
Other controls						X
Observations	13,753	1,459	12,294	12,249	897	897

Each observation in an employee. The dependent variable is the difference in the log of the strike price received by the employee and its four-week moving average. Other controls include measures of personality type (e.g., introversion vs. extroversion) and self-reported work style. Standard errors are heteroskedasticity robust.

Table 4. Job satisfaction in September 2006 and initial stock option exposure

Dependent variable: Normalized satisfaction source

Job satisfaction aspect	Compensation & Benefits				Compensation & Benefits			
	All employees				Pre-IPO hires		Post-IPO hires	
Moving average used	4 week	9 week	13 week	26 week	4 week	26 week	4 week	26 week
Ln(MA) - Ln(Strike)	0.454 (0.326)	0.215 (0.297)	0.213 (0.239)	0.304** (0.145)	0.278 (0.452)	0.355** (0.181)	0.671 (0.575)	0.255 (0.241)
Ln(Initial salary)	-0.060 (0.062)	-0.060 (0.062)	-0.060 (0.061)	-0.058 (0.061)	-0.510** (0.212)	-0.472** (0.203)	-0.019 (0.065)	-0.018 (0.065)
Ln(Shares granted)	0.179*** (0.035)	0.178*** (0.035)	0.177*** (0.035)	0.175*** (0.034)	0.180 (0.134)	0.146 (0.126)	0.179*** (0.035)	0.178*** (0.034)
Fixed effects								
Quarter of hire*initial job grade	X	X	X	X	X	X	X	X
Observations	3,863	3,863	3,863	3,863	803	803	2,819	2,819
R-squared	0.136	0.135	0.136	0.136	0.108	0.110	0.138	0.138

Each observation is an employee who completed a job satisfaction survey in September-October 2006. The dependent variable is the normalized average response to questions about satisfaction with compensation and benefits. Standard errors are heteroskedasticity robust.

Table 5. Components of job satisfaction and initial stock option exposure

Job satisfaction aspect	Overall satisfaction	Comp and Benefits	Salary	Total comp over horizon			Benefits	Relative to competitors	Fairness
				1 yr	1-2 yrs	3-5 yrs			
Ln(26 week MA) - Ln(Strike)	0.171 (0.170)	0.304** (0.145)	0.061 (0.131)	0.467*** (0.142)	0.393*** (0.139)	0.362** (0.152)	0.395*** (0.150)	0.155 (0.172)	0.363** (0.176)
Ln(Shares granted)	0.098*** (0.034)	0.175*** (0.034)	0.109*** (0.034)	0.189*** (0.046)	0.132*** (0.037)	0.074** (0.038)	0.105** (0.048)	0.101** (0.047)	0.150*** (0.051)
Ln(Initial salary)	-0.011 (0.047)	-0.058 (0.061)	-0.045 (0.059)	-0.038 (0.057)	-0.058 (0.061)	-0.102* (0.061)	0.256*** (0.081)	-0.013 (0.052)	-0.047 (0.060)
Fixed effects									
Quarter of hire*initial job grade	X	X	X	X	X	X	X	X	X
Observations	3,872	3,863	3,428	3,420	3,419	3,407	3,421	3,404	3,401
R-squared	0.096	0.136	0.133	0.143	0.125	0.098	0.067	0.120	0.146

Each observation is an employee who completed a job satisfaction survey in September-October 2006. The dependent variable is the normalized average response to a all questions (column 1), to all questions on compensation and benefits (column 2), or to specific questions about satisfaction with various aspects of compensation and benefits (columns 3-9). Standard errors are heteroskedasticity robust.

Table 6. Stock option exposure and subjective performance

Dependent variable: normalized quarterly subjective performance (Perf) score

Employee start date	All employees		Pre-IPO	Pre-IPO	Pre-IPO	Post-IPO
Time period included	Full	Full	Full	Pre-IPO	Post-IPO	Post-IPO
Moving average used	4 week	26 week	26 week	26 week	26 week	26 week
LnDelta(Strike) - LnDelta(MA)	0.099 (0.065)	0.029 (0.067)	0.277 (0.253)	0.192 (0.266)	1.070** (0.484)	-0.041 (0.078)
Ln(Shares granted, at moving average delta)	0.086*** (0.019)	0.093*** (0.019)	0.057 (0.047)	0.210** (0.104)	0.035 (0.054)	0.106*** (0.022)
Ln(Initial salary)	-0.010 (0.018)	-0.014 (0.019)	0.058 (0.069)	0.130 (0.299)	0.072 (0.077)	-0.024 (0.020)
Fixed effects						
Quarter of hire*initial job grade	X	X	X	X	X	X
Quarters since hire	X	X	X	X	X	X
P value for equality of first 2 coefficients	0.871	0.429	0.415	0.951	0.052	0.126
Observations	82,241	82,241	19,326	1,220	17,107	62,915
R-squared	0.059	0.059	0.044	0.029	0.047	0.065

Each observation is an employee*quarter. The dependent variable is an employee's quarterly performance evaluation score, normalized with job grade (i.e., job track and level combinations) and quarter combinations. Standard errors cluster for month in which employee was hired. The full time period is 2003Q1 to 2008Q1. The pre-IPO time period is defined as 2003Q1-2004Q2; the post-IPO time period is 2004Q4 to 2008Q1 (the IPO was on August 19, 2004).

Table 7. Summary of employee activity data

Activity measures by group	Average # per workday	Percent done by group	Data range	
			From	To
Software engineers (SWEs)				
Code reviews (as author or reviewer)	1 to 2	81%	Jan 2006	Jun 2008
Bugs database actions	1 to 2	45%	Jan 2006	Jun 2008
Builds	20 to 50	92%	Jun 2007	Jun 2008
Perforce (p4) calls (max 1 per 5-second period)	50 to 100	47%	Jan 2006	Jun 2008
Wiki page edits	0.5 to 1.0	44%	Jan 2006	Jun 2008
Wiki page views	5 to 10	51%	Jan 2006	Jun 2008
Interviews	1.0 to 2.0	30%	Jan 2006	Jun 2008
Hours with Perforce call or wiki usage	5.2		Jan 2006	Jun 2008
Online sales and operations staff (OSOs)				
Customer service rep (CSR) emails	2.0 to 5.0	21%	Jul 2004	Jun 2008
Ad approvals (max 1 per 5-second period)	100 to 200	53%	Jul 2004	Jun 2008
ICS page views	200 to 500	40%	Jul 2004	Jun 2008
Hours with ICS page view or CSR email	7.1		Jul 2004	Jun 2008

Notes: For the purposes of our analysis, software engineers include those in the software engineering job track (T) in Engineering, Operations, or Sales, excluding managers and directors and those with project, product, or hardware in their title. Online sales staff includes jobgrades E1 and N2 in the Sales department in AdWords, AdSense, or Checkout operations.

Precise activity measures per workday have been redacted. We hope to share more in a later version.

Table 8. Normalized activity measures, by subjective performance decile

Panel A. Software Engineers

Decile	Perf score	Hours	Normalized activity measures							
			Code revs (Auth)	Code revs (Rev)	Bug actions	Builds	P4 calls	Wiki edits	Wiki views	Interviews
Top	4.04	0.17	0.36	0.51	0.34	0.08	0.01	0.15	0.05	0.31
9	3.80	0.14	0.31	0.37	0.24	0.11	0.01	0.07	0.01	0.20
8	3.67	0.06	0.19	0.20	0.09	0.10	0.03	0.06	-0.01	0.11
7	3.56	0.08	0.08	0.11	0.07	0.04	0.05	0.03	-0.03	0.11
6	3.48	0.05	0.05	0.03	0.01	0.10	0.00	0.02	-0.02	0.04
5	3.41	-0.02	-0.01	-0.04	-0.04	0.06	-0.01	-0.01	-0.04	0.02
4	3.34	-0.04	-0.08	-0.11	-0.09	-0.03	-0.02	-0.02	-0.03	-0.05
3	3.29	-0.04	-0.13	-0.18	-0.11	0.01	0.02	-0.04	-0.03	-0.08
2	3.19	-0.08	-0.19	-0.25	-0.15	-0.04	-0.03	-0.06	0.00	-0.16
Bottom	3.00	-0.17	-0.30	-0.35	-0.19	-0.17	-0.05	-0.10	-0.07	-0.22
Correlation		0.097	0.200	0.256	0.161	0.067	0.021	0.068	0.028	0.149

Panel B. Online Sales and Operations (OSO) staff

Decile	Perf score	Hours	Normalized activity measures		
			CSR emails	ICS page views	Ad approvals
Top	3.88	0.14	0.21	0.04	0.08
9	3.65	0.15	0.23	0.09	0.16
8	3.52	0.15	0.14	0.10	0.10
7	3.31	0.24	-0.02	0.34	0.30
6	3.32	0.07	-0.11	0.09	0.16
5	3.32	0.20	0.15	0.09	-0.05
4	3.07	0.07	0.07	0.05	-0.07
3	3.02	-0.01	-0.06	-0.03	-0.05
2	3.02	-0.04	-0.09	0.03	-0.04
Bottom	2.89	-0.10	-0.14	-0.10	-0.10
Correlation		0.066	0.100	0.032	0.064

Activity measures are normalized and Perf score deciles are formed within job grade*quarter combinations. Correlations are between normalized performance ratings and normalized activity measures.

Table 9. Activity measures and performance ratings

Dependent variable: Normalized performance rating

Independent variable: Normalized activity measures

Employee type	Software engineer		OSO staff	
Sample period	1/06 - 6/08		7/04 - 6/08	
Hours with activity	0.029*** (0.011)	0.029** (0.011)	0.022 (0.023)	-0.015 (0.023)
Code reviews (as author)	0.033*** (0.013)	0.029** (0.012)		
Code reviews (as reviewer)	0.182*** (0.013)	0.156*** (0.013)		
Bug actions	0.060*** (0.011)	0.055*** (0.011)		
P4 calls	-0.006 (0.010)	-0.007 (0.010)		
Wiki page views	-0.008 (0.013)	0.026** (0.013)		
Wiki page edits	0.037*** (0.012)	0.016 (0.012)		
Interviews conducted	0.121*** (0.009)	0.089*** (0.009)		
CSR emails			0.078*** (0.018)	0.087*** (0.017)
ICS page views			-0.039 (0.023)	0.016 (0.023)
Ad approvals			0.109*** (0.019)	0.137*** (0.020)
Fixed effects				
Quarter*Job Grade	X	X	X	X
Quarters since hire		X		X
Observations	24,020	24,020	5,631	5,631
Unique employees	4,459	4,459	1,782	1,782
R-squared	0.088	0.109	0.012	0.066

Each observation is an employee*quarter. All measures are normalized within jobgrade*quarter combinations. Standard errors cluster on employees.

Table 10. Stock option exposure and performance/activity measures -- software engineers (SWEs)

Employees included	All	SWE	SWE	SWE	SWE	SWE	SWE	SWE	SWE	SWE	SWE	SWE	SWE
Normalized performance measure	Perf	Perf	Predicted Perf	Hours	Code rev (A)	Code rev (R)	Review time	Bug actions	Buils	P4 calls	Wiki edits	Wiki views	Interviews
LnDelta(Strike) - LnDelta(MA)	-0.030 (0.082)	-0.100 (0.142)	-0.042 (0.039)	-0.158 (0.172)	-0.133 (0.208)	-0.162 (0.197)	0.023 (0.103)	-0.237*** (0.088)	0.256 (0.245)	-0.195* (0.106)	-0.048 (0.124)	0.147 (0.135)	0.009 (0.166)
Ln(Shares granted)	0.094*** (0.021)	0.086*** (0.028)	0.016* (0.009)	0.045 (0.038)	0.112*** (0.040)	0.119*** (0.041)	-0.018 (0.018)	0.028 (0.021)	0.074* (0.046)	-0.020 (0.026)	-0.041 (0.026)	-0.013 (0.032)	-0.087*** (0.029)
Ln(Initial salary)	-0.008 (0.020)	0.048 (0.037)	0.026** (0.012)	0.100** (0.046)	0.178*** (0.042)	0.178*** (0.048)	-0.041 (0.028)	0.162*** (0.035)	0.199*** (0.058)	0.053 (0.033)	0.141*** (0.040)	0.109** (0.046)	-0.260*** (0.042)
Fixed effects													
Quarter of hire*initial job grade	X	X	X	X	X	X	X	X	X	X	X	X	X
Quarters since option grant	X	X	X	X	X	X	X	X	X	X	X	X	X
P value for equality of first 2 coefficients	0.202	0.25	0.198	0.285	0.293	0.213	0.714	0.008	0.500	0.117	0.957	0.289	0.995
Observations	69,122	23,262	27,952	27,952	27,952	27,952	27,952	27,952	15,454	27,952	27,952	27,952	27,952
R-squared	0.072	0.088	0.140	0.032	0.085	0.132	0.026	0.066	0.056	0.022	0.029	0.048	0.111

An observation is an employee*quarter. All performance measures are normalized within job grade*quarter combinations. "Predicted Perf" is an employee's predicted Perf score from their measured activity using the model in Table 9, column 2. Standard errors cluster for month in which employee was hired. The time period is 2006Q1 to 2008Q2, except that data for builds is available from Jun 2007 to Jun 2008.

Table 11. Stock option exposure and performance/activity measures -- online sales and operations (OSO) staff

Employees included	All	OSO	OSO	OSO	OSO	OSO
Normalized performance measure	Perf	Perf	Predicted Perf	Hours	CSR emails	ICS page views
Correlation of measure with Perf				0.0723	0.1014	0.0404
LnDelta(Strike) - LnDelta(MA)	0.012 (0.070)	0.246 (0.506)	-0.053 (0.038)	-0.137 (0.275)	-0.639*** (0.230)	-0.429* (0.252)
Ln(Shares granted)	0.092*** (0.020)	0.009 (0.161)	0.034*** (0.013)	0.102 (0.107)	0.204* (0.121)	0.075 (0.106)
Ln(Initial salary)	-0.014 (0.019)	-0.240* (0.124)	0.028*** (0.010)	-0.090 (0.075)	0.239** (0.118)	0.297*** (0.094)
Fixed effects						
Quarter of hire*initial job grade	X	X	X	X	X	X
Quarters since option grant	X	X	X	X	X	X
P value for equality of first 2 coefficients	0.34	0.72	0.085	0.523	0.011	0.15
Observations	81,021	2,055	2,408	2,408	2,408	2,408
R-squared	0.060	0.099	0.102	0.076	0.061	0.154

An observation is an employee*quarter. "Predicted Perf" is an employee's predicted Perf score from their measured activity using the model in Table 9, column 4. All performance measures are normalized within job grade*quarter combinations. Standard errors cluster for month in which employee was hired. The full time period is 2004Q3 to 2008Q1.

Table 12. Equity exposure and performance, by wealth quartile

Dependent variable: Normalized performance rating (within job grade*quarter)

Years after grant	All	Lowest	2nd	3rd	Top	Top decile
Specification #1						
LnDelta(Strike) - LnDelta(MA)	-0.129 (0.094)	0.043 (0.152)	-0.134 (0.139)	-0.529 (0.360)	-0.281 (0.182)	-0.210 (0.236)
Ln(Shares granted)	0.104*** (0.027)	0.131*** (0.036)	0.178*** (0.050)	0.109** (0.052)	0.029 (0.034)	-0.055 (0.059)
Ln(Initial salary)	-0.010 (0.024)	0.016 (0.036)	-0.106*** (0.032)	-0.057 (0.042)	0.054 (0.055)	-0.205 (0.205)
Specification #2						
Ln(Shares granted)	0.091*** (0.025)	0.053 (0.039)	0.177*** (0.053)	0.106** (0.052)	0.039 (0.032)	-0.042 (0.054)
Ln(Initial salary)	-0.024 (0.024)	-0.004 (0.035)	-0.108*** (0.034)	-0.058 (0.041)	0.036 (0.055)	-0.286 (0.198)
Fixed effects (both specifications)						
Quarter of hire*initial job grade	X	X	X	X	X	X
Quarters since option grant	X	X	X	X	X	X
Observations (both specifications)	50,892	10,235	13,592	13,607	13,458	5,320

Each observation is an employee*quarter combination. The sample is split according to employee's beginning of quarter Google-equity wealth, calculated as the immediate-exercise value of the employee's Google options plus the value of her restricted stock. The quartile cutoffs are approximately \$30,000, \$70,000, and \$200,000; the cutoff for the top decile is approximate \$1,000,000. Standard errors cluster on month in which the employee was hired. The sample period is 2004Q4 to 2008Q1.

Table 13. Vesting, sales/exercise, and performance

Dependent variable: Normalized performance rating or activity (within job grade*quarter)

Employees	All	All	All	All	SWE	SWE	SWE	SWE	SWE
Dependent variable	Perf	Perf	Perf	Perf	Perf	Predicted Perf	Hours	Reviews (A)	Reviews (R)
Ln(Shares granted)	0.098*** (0.014)	0.100*** (0.014)	0.099*** (0.014)	0.100*** (0.014)	0.092*** (0.021)	0.015* (0.009)	0.042 (0.035)	0.102*** (0.038)	0.119*** (0.039)
Ln(Initial salary)	-0.030* (0.016)	-0.032** (0.016)	-0.031* (0.016)	-0.032** (0.016)	0.044** (0.038)	0.029** (0.012)	0.097** (0.045)	0.188*** (0.043)	0.176*** (0.048)
Ln[max(10%,% of shares not vested)]		0.005 (0.018)		-0.006 (0.027)	0.052 (0.041)	0.043** (0.020)	0.014 (0.039)	0.062 (0.047)	0.171*** (0.055)
Dummy for not vested < 10%		-0.274*** (0.043)		-0.266*** (0.048)	-0.220*** (0.076)	-0.077*** (0.020)	-0.192*** (0.067)	-0.358*** (0.056)	-0.457*** (0.081)
Ln[max(10%,% of shares not sold)]			-0.002 (0.018)	0.021 (0.028)	0.056 (0.044)	-0.001 (0.020)	0.006 (0.058)	0.019 (0.073)	0.048 (0.061)
Dummy for not sold < 10%			-0.032 (0.038)	-0.043 (0.050)	-0.142* (0.074)	-0.041 (0.028)	0.013 (0.073)	0.082 (0.118)	-0.061 (0.110)
Fixed effects									
Quarter of hire*initial job grade	X	X	X	X	X	X	X	X	X
Observations	90,357	90,357	90,357	90,357	32,880	32,880	32,880	32,880	32,880
R-squared	0.045	0.046	0.045	0.046	0.057	0.111	0.032	0.070	0.104

Each observation is an employee*quarter combination. At the beginning of each quarter, the share of equity in an employee's original grant that are unvested and that have not been exercise or sold is calculated. The Ln(unvested) and Ln(unsold) variables have a minimum of Ln(0.1); the indicator variables are turned on below these levels.

Standard errors cluster on month of initial hire.

Table 14. Equity exposure and performance, by time since initial grant

Dependent variable: Normalized performance rating (within job grade*quarter)

Years after grant	All	First 4	First	Second	Third	Fourth	Fifth+
Ln(Shares granted)	0.092*** (0.016)	0.095*** (0.016)	0.123*** (0.015)	0.095*** (0.020)	0.074*** (0.021)	0.046 (0.042)	-0.045 (0.068)
Ln(Initial salary)	-0.027 (0.016)	-0.029* (0.016)	-0.061*** (0.017)	-0.013 (0.026)	-0.025 (0.030)	0.090 (0.075)	0.267** (0.113)
Fixed effects							
Quarter of hire*initial job grade	X	X	X	X	X	X	X
Quarters since option grant	X	X	X	X	X	X	X
Observations	90,413	88,552	34,843	31,782	14,978	5,993	1,861
R-squared	0.067	0.067	0.094	0.046	0.066	0.101	0.171

This table replicates Table 13, column 1 in the first column and the reestimates the model including employees with a certain amount of time since their initial options grant.

Table 15. Promotion, performance, and activity

Dependent variable: Normalized performance rating (within job grade*quarter or job grade*year)

Employees included	All	SWEs	All	SWEs	SWEs	SWEs	SWEs	SWEs	SWEs	SWEs	SWEs
Dependent variable	Perf	Perf	Perf	Perf	Predicted Perf	Reviews (A)	Reviews (R)	Bug actions	Wiki edits	Interviews	Hours
Prior promotion?											
Quarter t-1	-0.535 (0.018)	-0.675 (0.027)	-0.556 (0.019)	-0.685 (0.025)	0.088 (0.009)	0.240 (0.038)	0.366 (0.038)	0.245 (0.038)	0.110 (0.031)	0.002 (0.034)	0.131 (0.032)
Quarter t-2	-0.356 (0.027)	-0.396 (0.052)	-0.337 (0.025)	-0.391 (0.050)	0.066 (0.012)	0.292 (0.041)	0.308 (0.043)	0.221 (0.043)	-0.013 (0.033)	-0.073 (0.041)	0.200 (0.035)
Quarter t-3	0.154 (0.025)	0.138 (0.053)	0.111 (0.024)	0.120 (0.051)	0.081 (0.013)	0.297 (0.059)	0.353 (0.056)	0.161 (0.066)	0.094 (0.064)	-0.026 (0.036)	0.053 (0.047)
Promotion quarter?			0.177 (0.008)	0.207 (0.012)	0.007 (0.003)	0.034 (0.012)	0.051 (0.012)	0.049 (0.012)	-0.033 (0.016)	0.028 (0.013)	0.040 (0.016)
Promotion quarter*prior promotion (t-2 ot t-3)	-0.224 (0.036)	-0.244 (0.086)	-0.180 (0.035)	-0.230 (0.087)	-0.012 (0.020)	0.028 (0.090)	-0.035 (0.087)	0.058 (0.066)	-0.101 (0.066)	-0.043 (0.055)	-0.198 (0.053)
Ln(Shares granted)	0.017 (0.022)	0.044 (0.030)	0.020 (0.022)	0.044 (0.030)	0.017 (0.009)	0.095 (0.033)	0.095 (0.034)	-0.003 (0.026)	-0.020 (0.027)	-0.010 (0.022)	0.010 (0.033)
Ln(Initial salary)	-0.016 (0.031)	-0.135 (0.065)	-0.019 (0.031)	-0.133 (0.066)	-0.005 (0.015)	0.032 (0.055)	0.008 (0.063)	0.104 (0.054)	0.162 (0.044)	-0.170 (0.040)	0.002 (0.054)
Fixed effects											
Quarters since hire	X	X	X	X	X	X	X	X	X	X	X
Job grade*quarter	X	X									
Job grade*year			X	X	X	X	X	X	X	X	X
Quarter of year			X	X	X	X	X	X	X	X	X
Observations	53,453	15,780	53,463	15,780	15,932	15,940	15,940	15,940	15,940	15,940	15,940
R-squared	0.034	0.051	0.046	0.067	0.028	0.025	0.038	0.012	0.010	0.029	0.015

Each observation is an employee*quarter combination. "Predicted Perf" is an employee's predicted Perf score from their measured activity using the model in Table 9, column 2. Dependent variables are normalized within job grade*quarter combinations in columns 1 and 2 and within job grade*year combinations in the other columns. Standard errors cluster on quarters.